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AND THE
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BY

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DURING the last six months and more the profession has been much surprised at seeing in prominent parts of lay periodicals certain startling telegrams to the effect that Surgeon-Colonel Lawrie, of Hyderabad, has discovered that Laveran's bodies are not at all of a parasitic nature as is held by the rest of us. Since Laveran's discovery has now been made for sixteen years; since it is supported by numerous observers all over the world; since the life-history of the parasite within the human body, its structure, its various species, and their connection with the different types of fever, have all been worked out with the greatest exactness and care; since similar parasites have been found in birds, tortoises, frogs, lizzards, &c.; and since, according to Thayer and Hewetson's Bibliography,

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between three and four hundred books, pamphlets and essays have been published on these subjects, almost all of them confirmatory of Laveran's theory, our surprise at the said telegrams is, I think, not to be wondered at. Unfortunately they have had a worse effect than to surprise us. Being published in the lay press, where every one could read them, and being necessarily so curt that we could neither understand them nor reply to them in a proper manner, the public has been largely induced to believe that we really could not reply to them at all; that the parasite is a mere illusion; and that Surgeon-Colonel Lawrie has, if I may use a metaphor, single-handed routed and rolled in the dust a crowd of bald-headed professors, whom overstudy has driven into the imbecility of believing in such a creature. Such (at least as I gather it from conversation with non-professional friends) is the general impression. And it appears to me highly undesirable that such an opinion should ever be allowed to gain ground at a moment like the present, when so much work remains to be done in reference to the subject.

It is a matter, then, for some congratulation, that Dr. Lawrie has detailed his views and reasons in his lecture delivered at Bombay on the 6th April last; we are at last in possession of his arguments in favour of his theory. As those arguments are entirely erroneous, and as it has been suggested to me by one of our Secretaries that a reply is called for, I shall venture to take up the time of the meeting by offering a few remarks on some of the more salient points of the lecture.

Before doing so, however, I hope that I may be allowed to give my own experience in regard to the parasite; a bitter and yet a fortunate ex-

perience ; and experience common to many who have once denied the existence of the parasite, or who have made mistakes in reference to it ; an experience which I believe Surgeon-Colonel Lawrie himself is bound to suffer from or to rejoice in before very long. It is this. Some years ago I began to look for the parasite without having any particular knowledge of the ordinary microscopic appearance of the blood—a subject requiring some study. I obtained some of Laveran's works and read all the literature which I could find about the hæmatozoon ; but I could discover nothing in malarial blood which accorded exactly with Laveran's descriptions, though there were many things there which tallied with them more or less, but which I shortly perceived were appearances common to all blood—mere *post-mortem* changes, and so on. At the same time, articles on the subject were appearing, written by two gentlemen, one of whom was serving under Surgeon-Colonel Lawrie at Hyderabad. It was not difficult to see that both these gentlemen were describing, as forms of the malaria parasite, some of the *simulacra* abovementioned. On being myself ordered to Secunderabad I took the opportunity of calling upon one of them, who showed me, as I had suspected, not a parasite, but what I knew well enough was a *simulacrum*. The result may be imagined. I began to disbelieve the whole theory ; I thought that Laveran, Golgi and Marchiafava had been deceived, like the gentlemen referred to, by *simulacra* ; and I even ventured to write articles (as Dr. Lawrie now writes) accusing such men as the above of mistaking crenated corpuscles for hæmatozoa!—implying, in a manner which really reflected my own ignorance, that some of

the most eminent Microscopists in Europe were ignorant of the common appearances of the common elements of the blood ! Fortunately I was soon to be undeceived. Going to England, I submitted my precious articles, together with a number of photomicrographs which had cost me about a year's labour, to Dr. Patrick Manson. He said the kindest things possible about my work, but — showed me a malarial crescent. I was bound to confess that, in spite of my studies, I had never seen such a thing before ; I knew at once that here was something quite foreign to normal blood (for by this time I had made a considerable study of blood) ; and I recognised that many months of my life had been wasted in a foolish and immature attempt to disprove what better men had proved.

I hope then that my own history of mistakes made and labour thrown away, and my experience of *both sides of the question* may be held to give me some right to discuss Surgeon-Colonel Lawrie's errors. And I think that our cases are very parallel. For instance, he says of some excellent diagrams of the malaria parasite drawn after certain eminent authors that "they are caricatures of vacuoles and crenating blood cells." Certainly vacuoles and crenated corpuscles have been mistaken for parasitic forms by beginners ; but does Surgeon-Colonel Lawrie really suppose that men like Marchiafava and Mannaberg could make such a juvenile mistake ? Does he really imagine that Manson does not know what a red corpuscle is like ? It is as absurd to think so as it would be to think that Surgeon-Colonel Lawrie himself does not know the difference between a boil and a cancer. Again, I think it very likely that he has been shown *simulacra* instead of the real animal, has had the acumen to detect the

error, and has jumped to the conclusion that the whole theory is untenable.

But here the parallel between our cases ends. I did not see the parasite at all, or did not recognise it, until Manson showed it to me; and I thought that the whole theory was founded on a misinterpretation of certain ordinary appearances in the blood. But Dr. Lawrie has, I believe, to judge from his lecture, seen one or two forms of the parasite, though he has not, evidently, devoted sufficient attention either to the literature of the subject, or to those forms of Laveran's bodies which most convince us of their parasitic nature. He has seen some of the bodies, but he has not sufficiently studied all of them to grasp their true significance; and, in order to explain the existence of those which he *has* seen he has built up a theory which is quite inconsistent with those which he *has not* seen. I hope that we shall both be excused in the day of judgment.

I say that he has not paid sufficient attention to the current literature of the subject, and has not studied carefully those forms of Laveran's bodies which demonstrate their parasitic nature--I mean the small amœboid bodies and the sporulating and flagellate forms. This is quite clear from the internal evidence of his lecture. For instance, nothing proves the parasitic nature of these bodies with more absolute certainty than the absolutely certain fact that they sporulate. The point is certified by scores, if not hundreds, of observers—I for one am prepared to swear to it. Golgi and the Italians have studied the whole matter with the utmost care; yet Lawrie allows himself to say that "no evidence has been adduced to show that Laveran's bodies sporulate." In the face of such an assertion we can only gasp and wonder whether he has ever given himself

the trouble to read the ordinary text-books, much less to study an ordinary quartan fever. Personally, I don't think that he can ever have seen a common rosette form. Yet, since the point is one of vital importance to his theory, I think that he should have had something to say about it more to the point than the few futile remarks about want of evidence. There is the best evidence in the work—that of one's own eye-sight.

Again I wonder whether he has ever seen an ordinary small amœboid body which, with its rapid amœboid movements within the corpuscle, is so suggestive of independent life. I doubt it, because in his remarks on some illustrations of the parasite given in the *British Medical Journal* he complains that "the representations of the three different varieties of 'parasite,'—the Quotidian, Tertian, and Quartan of the first part of the first day are all exactly alike. Had he studied both the literature on the small amœboid bodies and the bodies themselves, he would have perceived the reason why the diagrams give them as all alike—because Nature has made them so.

Thirdly, what about the flagella (or flagellated spores as Manson has described them)? It was these bodies which first convinced Laveran that he was dealing with a parasite; and I will venture to say with the utmost emphasis that any one who has seen them and looked properly at them and watched them would *laugh* at the idea that they are anything but parasitic. If these bodies are not endowed with an independent life, then eels, snakes, and worms are dead creatures; if these are not parasitic, trypanosomes are not parasitic. I do not think that Surgeon-Colonel Lawrie has ever seen them at all, or, if he has seen them, has ever watched them. He seems to think that they are the pseudopodia of

leucocytes, as I thought they were before I had seen them, and talks of white cells being able to throw out "pseudopod processes, *which if thread-like or filamentous are called cilia or flagella*" (a thing by the way that has not been heard of before). Fancy the pseudopodium of a leucocyte wriggling about from field to field for some hours with a movement compared with which those of a serpent or an eel would be slow! It is almost impossible to conceive that any one who has observed flagella could mistake them for any normal cell or process of a normal cell; and that Surgeon-Colonel Lawrie has fallen into an error of such magnitude is enough to prove, to any mind, that his remarks on Laveran's bodies are not founded upon any very intimate acquaintance with them.

Besides views and theories which are open to grave criticism, Dr. Lawrie's lecture contains several startling statements of a kind which we must take exception to. I mean that he often gives out such and such as an accepted and notorious fact, thus inducing those who have no special knowledge of the subject to believe it; while, in reality, it is not a fact at all and is accepted by no one. His statement to the effect that there is no evidence to prove sporulation is one of these; another is his declaration that Laveran's bodies "are not present in the blood in a considerable proportion of cases of malarious fever of the most fatal and chronic types. They cannot, therefore, be the cause of the fever." One would think by the bold manner in which this statement is made that it must be quite undeniably true; so that the deduction from the premiss, namely, that the bodies cannot therefore be the cause of the fever, seems to follow rationally enough. As a matter of fact, the

premiss is the opposite of the truth; Laveran's bodies are present in every case of malarial fever, as every one who has studied the literature of the subject knows well enough. It is unnecessary to knock down again this old dummy of an argument which has been knocked down already dozens of times, but which is continually being set up again by some *à priori* sceptic. For instance, Thayer and Hewetson have lately found the parasite in every one, except two or three convalescent cases, out of 333 cases treated as in-patients in the John Hopkin's Hospital. A third example of this kind of statement by Surgeon-Colonel Lawrie is that in which he describes intra-cellular swarming of pigment as that fact "on which the plasmodists rely most confidently for the identification of Laveran's bodies as parasites." He goes on rightly to say that the argument must be useless since swarming is seen also in leucocytes. But whoever identified Laveran's bodies as parasites on such grounds as this? No one ever dreamed of doing so that I remember. It is characteristic of the lecture that Dr. Lawrie appears to think that swarming in the human leucocyte was discovered by him, and that, owing to the same, Laveran's bodies must be a kind of leucocyte. Now, I suppose swarming was discovered when the first good microscope was made; and if it be characteristic of leucocytes, then common amœbæ, gregarinæ, and I daresay some hundreds of cells, must be leucocytes. Swarming is a very common property of many cells, especially when in a dying condition; I described it in *blood-plates* some years ago. The plasmodists have never thought of relying on such a rotten reed as this for proof of the parasitic nature of Laveran's bodies; they rely on sporulation, on the flagella,

and on numerous other reasons which I do not think Surgeon-Colonel Lawrie has considered deeply enough.

We may criticise almost every view propounded by Dr. Lawrie. Has there ever been an argument containing less scientific instinct than the following:—"If Laveran's bodies," says Dr. Lawrie, "were the cause of the fever of malaria, the plasmodists would surely have been able before now to isolate them in pure culture, and to have demonstrated their existence outside the human body." I do not follow the logic; in other words, it amounts to this, that nothing can be a parasite which cannot be cultivated in pure culture. Why then, round worms, and thread-worms, and all the entozoa, and the surra-parasite, and perhaps the variola bacillus (if there be one), and perhaps a score of other organisms, are not parasitic. Is there any peculiar quality in a parasite that should always make it cultivable in gelatine? Or, cannot an animal exist within the human body until we have demonstrated its existence without? Why, we are not able to cultivate trypanosomes; but the wildest sceptic would not deny their parasitic nature. Then look at Surgeon-Colonel Lawrie's argument based on the intravenous injection of malarial blood. He did not try it on men, but on dogs and monkeys; it failed, and hence he argues the disease is not communicable in this way. Of course, numerous experimenters have long ago shown that injections fail from men to animals, but succeed from men to men. The only experiment of the kind which was likely to succeed was the one which was not tried by Dr. Lawrie—a fact of which he appears to be quite ignorant. Lastly, consider his experiment with the liver and spleen of

frogs. These animals live in malarious places, but he cannot find their spleens and livers to be diseased. Hence, he argues, they are immune from malaria; hence their spleens, livers and serum should have an antitoxic effect with malaria patients. He tried it on one patient (only one, be it observed), and the treatment had no effect. But really, what could he expect? Frogs do not, I believe, have cholera, though I suppose they live in cholera-infected pools. Would their liver and spleens be antitoxic against cholera for that reason. Buffaloes do not suffer from influenza, I fancy, but no one would think that their serum is therefore calculated to be an antidote to that disease. I cannot find that such arguments as these have any bearing at all on the point at issue.

It appears almost unnecessary to discuss Lawrie's alternative theory respecting the nature of Laveran's bodies, namely, that they are immature white corpuscles, evolved in the spleen and allowed to drift into the general circulation when that organ is diseased. The theory is quite impossible for the following reasons:—

- (a) If it were true, we must expect Laveran's bodies to be found in all cases of spleen-disease. As a matter of fact, they are found only in cases of malaria.
- (b) If Laveran's bodies be young leucocytes (which Lawrie thinks are derived from nuclei of red corpuscles) we should be able to observe them, or bodies like them, in all *post-mortem* examinations of the spleen; where it should be presumed, the evolution of leucocytes is continually going on. But whoever

finds anything like Laveran's bodies in healthy spleens ?

(c) Dr. Lawrie's whole speculation regarding the origin of leucocytes from the nuclei of red cells is not only advanced by him without even the elements of proof, but is contrary to all existing theory.

(d) How can his theory explain the black pigment so characteristic of the malaria parasite and so entirely different from the granules of leucocytes ? What leucocyte ever took on the form and structure of a crescent ; or has been known to sporulate ; or to give forth flagellated spores ?

(e) Dr. Lawrie considers that Laveran's bodies are due to a kind of malversion, induced by diseased conditions of the spleen, of the ordinary process (as he puts it) of evolution of leucocytes from the nuclei of red corpuscles. But he must really "explain his explanation" a little further, since such an extraordinary malversion would itself be as extraordinary as the original phenomenon. His theory does not carry us really a single step from the inexplicable nearer to the explicable.

With respect to his remarks on phagocytosis, in a witty passage he laughs at the accepted theories on this point and pictures to us the "disappointed macrophage" with "all his obliging endeavours to benefit the patient rendered nugatory by his own amiable and well-intended greediness." This is very amusing,

but it necessarily destroys our belief in any scientific value of the lecture, because it assures us that Surgeon-Colonel Lawrie has never even seen phagocytosis—a phenomenon which may be witnessed in any fair specimen containing flagellum-cysts.

After all is said, however, I think that Surgeon-Colonel Lawrie has certainly seen two or three of the more evident forms of the parasite, namely, crescents, crescent-spheres and the larger intra-corpuscular forms of intermittent fevers. I doubt whether he has seen, I certainly think that he has not studied, the rest.

Any way he has advanced considerably further than I had advanced when in the same stage of scepticism, and any one who is inclined to blame him must severely censure me. In fact this is why I have allowed myself to criticise his remarks freely—because I do not think that any one has any intention to blame either of us—it is scarcely worth the trouble. Indeed I consider that those who have had the energy (or rashness) to tackle such a tough subject without previous instruction from one who is acquainted with the parasite is always to be patted on the back, and that the more perhaps, the heavier are the falls he receives in the encounter. Men of science, I fancy, are rather inclined to look upon sceptics in the matter as the late Poet Laureate looked upon those

.....Petty fools of rhyme
Who shriek and sweat in pigmy wars
Before the stony face of time,
And looked at by the silent stars,

and to let us talk our fill until we are converted to more authentic views. The malaria parasite is something like Ibsen's Great Boyg, which the cleverest Peer Gyat of us all can scarcely

get over. It exists; it is always there; it is always a parasite, say what we will.

P.S.—Since writing the above I have seen brief letters by Manson and Thin in the *British Medical Journal* on the subject. It will be observed that some of the criticisms coincide with the above. I hope it will not be considered that Surgeon-Colonel Lawrie's views do in any way represent those of the profession in India.
